



Reprinted from 'Impact of Space Exploration
on Society' Volume 8, Science and Technology
Series, 1966, a publication of the American
Astronautical Society. Publications Office
P.O. Box 746, Tarzana, California 91356

PLANETARY EXPLORATION AND BIOLOGICAL RESEARCH

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Space flight has had very little impact on biology so far. Dr. Harold P. Klein himself is associated with some outstanding exceptions: The support that space research has given to the problems of organic molecular origins. However, fundamental biology has so far been such a negligible part of the actual commitments of the space program that we should not delude ourselves with any other expectation.

The picture is changing, especially with the dramatic steps of the planetary programs. But we have had only the first glimpse and this needs to be followed up by much closer study and frequent visits to reconnoiter Mars, Venus, and Jupiter. Every celestial body has some importance to biology, just as the subject merges with cosmology in its concern with the origins and evolution of large scale bodies.

It is grossly premature to draw any fixed conclusions about the history of Mars from the Mariner IV Photographs. They have an enormous amount of information tucked away on them, only bit by bit coming to the surface. One point does seem clear - that Mars has a great deal of fine structure, requiring much higher resolution. One of the most important questions these probes have to answer is the distribution of temperature and moisture. There is still every possibility of quite a good deal of subsurface water on Mars as ice or as permafrost, which is exactly where we would have to expect it to be in view of what we have long known about the average crustal temperature of the planet. So here is a geological question, but one of the most urgent for biology.

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NASA has not enjoyed a very satisfactory rapport with many earth-bound biologists. This is perhaps inevitable because the grounds of communication at this stage of the program are necessarily so slim. By the very tradition of their work it is also true that biologists must have a more parochial (that is, more earth-limited) outlook on science than physicists and chemists who already know that their subjects are relevant to cosmic nature. Of course the change in this outlook is exactly one of the major opportunities for the exploration of space, but we can hardly expect that iconoclastic approaches will be welcomed by the old order, especially when they cost, or they seem to cost, a great deal of money.

I would like to press a constructive suggestion for this and many other reasons. Now that we have passed the first pioneering stage, we should cast our programs to match a longer range pattern. Instead of selecting isolated experiments for implementation, NASA's principal efforts should be to build general purpose laboratories, reprogrammable from earth, so that the whole scientific community can have a continuing opportunity to experiment in space and on the planets. The astronomical observatories are in many ways a prototype of this approach. One implication is that NASA will choose participants willing to regard themselves as architects, whose design will be in the service of the community. This has not been the main pattern of space science to date. And this has tended to disassociate it from the important traditions of a broad scientific interface and of experimental competition and criticism which are fundamental to real advance.

An important theme of this meeting should be the impact of technology rather than of "space". I suggest that NASA's charter be broadened to help cover some of the glaring breaches in the application of technology to biological questions. This approach seems to me essential because of the very powerful momentum that the very existence of an organization

like NASA implies. There are important reasons at many levels, and most of these are defensible in most idealistic terms, why the budget of such an organization should remain relatively stable from year to year with allowances for temperate growth, once its major organizational requirements have been laid out.

But this approach leaves no room for flexibility in looking for new goals, for attempting to harmonize the most fruitful application of these powerful technological resources. You all know the vicious cycle that it takes a rather large investment to plan a program in the necessary detail so that it can attract the funding needed to get it off the ground. With NASA committed by charter to this one use of its powerful technology, it has no basis by which it can explore others that its own staff might wish to consider.

It seems to me that NASA already touches almost every base in science and technology for its pioneering purposes; it could very well be charged to do at least the system definition planning for many other technological projects and, indeed, to seek out just the ones that seem most propitious and would give the greatest cost effectiveness over a range of human values. Space research and development is already doing this by indirection to an extraordinarily extent. But we need an aggressive and capable agency to shoulder this job over a much broader front of non-military applications.

I am sure many of you could furnish you own favorite suggestions for constructive technological development. One that I feel that is long overdue, because it ought to be quite amenable to the kind of technology that NASA knows how to mobilize, is the artificial heart. After all, this is a pump of high reliability, good work-to-weight ratio, effective restart capability, which circulates fuel and oxidants under computer control. Should not space technology include a handful of small projects like this to help fill its mission gap over the next decade?

If you stop to think of this as an example, or to take another example, if you look for computer applications in biology, and then look further at the contrast between the potential impact of technology and what we actually have, you might agree with my outlook in answering the question of this symposium: the impact of space technology on biology is to show us by dramatic example how much is possible that we could do if we would just face up to our unmet challenges.